Reply to Prof. Gregory Pasternack

Dear Prof. Gregory Pasternack,

We thank you for your comments, which will help improve the clarity of the manuscript as well as the choice of the methods.

According to both reviews we decided to make very substantial changes to the paper. This work is a methodological study that introduces relatively new wavelet analysis tools in the field of geomorphic analysis (namely, Wavelet Ridge Extraction), in order to identify the pseudo-periodicity of alternating morphological units from a general point of view (and not only pool-riffle morphology). We did initially introduce an index method as a benchmark, but this index was poorly designed due to a lack of physical basis for the choice of the variables. We also neglected some relevant literature on the identification of the morphological units using DEMs, which could be used as benchmark methods in this paper.

For that, we suggest changing the title of this paper to "***Automatic identification of alternating morphological units in river channel using wavelet analysis and ridge extraction***". This will be more general and focuses on the method and not on the pool-riffle morphology.

We have presented two methods in this article. The first one is the wavelet method which represents alternating morphological units (pools and riffles) as pseudo-periodic signals with a continuous wavenumber function K(x). The other one is the index method which is a benchmark method that gives a discrete identification of the morphological units.

According to your suggestions, we will cut out the index method and replace it with an existing method "Mesohabitat Evaluation Model (MEM)" inspired from Hauer et al. (2009). For that, we will focus only on the wavelet analysis and ridge extraction in the univariate and the multivariate cases and compare its results with two benchmark methods: BDT (O'Neill and Abrahams, 1984) to the bed elevation data and MEM (Hauer et al., 2009) to three variables (velocity, hydraulic radius, and bed shear stress).

We will also minimize the use of modeled variables and apply the methods directly on field measurements (velocity and hydraulic radius variables at the lowest surveyed water level). We will use modeling results (Fluvia model) for bed shear stress only, as the energy slope cannot be determined in a sufficiently accurate manner with the measurements.

For the literature, we missed many recent studies and methods in relation to this work. So first we will add a table that summarizes examples of methods of identifying these morphologies and the variables chosen to do that. Second, we will change and add many recent works especially those working with meter-resolution digital elevation models (DEMs). Finally, we will clearly state the objectives of this study in the abstract and in the introduction.

Another important thing is that we propose a new structure of the paper:

I- Introduction:

First, we will state the scope of this study with adding more fields of its application. Second, we will introduce a literature review of metrics, variables used to identify and characterize the alternating morphological units. We will focus on two kinds of numerical criteria computed at reach scale:

- The distribution of spacings between morphological units (mean, mode, etc.),
- After computing the mean values of geometrical and flow properties (velocity, hydraulic radius, bed shear stress, etc.) in each class of morphological units (e.g. pools, riffles, runs, etc.) we will evaluate the covariance matrix of these parameters.

II- State of art methods for a quantitative assessment of morphological variability within a reach:

We will present some recent methods and works in the identification of these alternating morphological units (pool-riffle in our case) and state their objectives and limitations. We will start with the Bedform Differencing Technique (BDT, O'Neill and Abrahams, 1984), which is simple but uses bed elevation as the sole variable, and relies on a tolerance criterion on elevation differences. We will then review index methods like Mesohabitat Evaluation Model (MEM, Hauer et al., 2009) which classify each position in the reach into a given discrete morphological unit (pool, riffle, run, plane bed, etc.). These methods rely on expert judgement to define the thresholds that define parameter classes. Finally, geostatistical methods (e.g. Legleiter, 2014) give a continuous description of river channel properties in spatially stationary way, using longitudinal and transverse variography. For these reasons, we are searching for a method that gives a continuous description of geometrical and flow characteristics along the reach with a non-stationary description.

III- Study objectives

We will state that this work aims to introduce relatively new wavelet analysis tools in the field of geomorphic analysis, the Wavelet Ridge Extraction, in order to identify the pseudo-periodicity of alternating bedforms from a general point of view. In this study we will use a dataset that presents mainly pool-riffle morphologies, but the method can be applied to any morphology.

We will present the scheme of the paper which include a methodological section of the wavelet analysis and ridge extraction in the univariate and the multivariate cases, a section that presents the comparison method (with defining more explicitly the two benchmark methods: BDT and MEM index), a discussion section, and conclusions.

IV- Data set and study reaches:

We will present the six reaches, more explicit information about data collection and about the numerical modeling (Fluvia), and the data that will be used in this study.

V- Wavelet method

1) Wavelet analysis and ridge extraction:

We will present a general introduction about wavelets including some methods such as the Wavelet Transform Modulus Maximum (WTMM, Gangodagamage et al., 2007) and other studies using the wavelets in the geomorphological field (Lashermes et al., 2007; McKean et al., 2009). Procedures such as the WTMM (Muzy et al., 1993) consist in extracting components of the signal, but they are not specifically designed to identify pseudo-periodic components in a univariate, let alone in a multivariate case. For this reason, we introduce the procedure called Wavelet Ridge Extraction (Lilly and Olhede, 2009).

2) Univariate case

We will present the methodology of this method in the univariate case using one of the three variables (velocity, hydraulic radius, and bed shear stress).

3) Multivariate case

We will present the methodology of this method in the multivariate case using the three variables (velocity, hydraulic radius, and bed shear stress).

VI- Results

1) Comparison method:

We will define more precisely the two benchmark methods: BDT and MEM index, and their classification of the morphological units (pool-riffle). We will define also the metrics and their computing method.

2) Application and results

We will present results of all methods for the six reaches and apply the comparison.

VII- Discussion

We will discuss results (longitudinal spacing, number of morphological units, etc.) with literature and with the two benchmark methods

VIII- Conclusions

Kind regards,

The authors

Comments:

It is unfortunate that the manuscript does not have continuous line numbering to aid reviewers and editors with referring to locations easily, even the repeating page numbers are only every 5th value, which is not convenient. Actually, based on page 8 where there is new numbering at the onset of section 4, I am totally confused as to how line numbering is done and it makes it harder to review the paper in a *discussion format that requires me to write out all my comments rather than simply mark up a manuscript. In future manuscripts, always include full and continuous line numbering.*

Response:

We used the Latex Template; maybe there is a problem in the numbering characteristics that we will modify in the revised version.

First 2 paragraphs of the introduction. It seems odd to me that the main reason why anyone should be interested in understanding the sub-reach variability of river topography is because of the potential application of such information to flood forecast modeling. Even in the applied realm that is only 1 of many applications that could be referred to. In my own research, the primary motivations are that such data is required for river design for a wide variety of purposes including river rehabilitation and enhancement and also because it informs fluvial ecohydraulics. In light of systemic global ecological collapse, these are more important to society than flood forecasting, in my professional opinion. At a minimum, I think the authors should identify a few more reasons why knowing topographic variability matters and add a citation for each. Also, of course, geomorphologists want to understand it in its own right as a basic scientific question that requires no justification, and of course it is also the case that this variability controls fluvial processes, so the lack of knowledge about it means that we really know little about processes; less than I think most people realize.

Response:

It's true that there are more reasons why knowing topographic variability matters like you mentioned above. For that we will modify the first paragraph of the introduction by adding examples of application of our study like the design of a synthetic river topography which is implemented in river restoration (e.g., Wheaton et al., 2004a), habitat modeling, ecohydraulics (e.g., Pasternack and Brown, 2013), and of environmental modelling (oxygen exchange, fish habitat) and also that this variability controls fluvial processes as sediment transport, but not focusing only on flood forecasting.

P. 2, lines 3-8. While this is generally true, the authors seem to be unaware that my lab group has already published theory and code that is the first to procedurally generate river terrains exactly to specification from the equations and parameters, and this methodology does include sub-reach-scale variability that can go to as high of a frequency as one wants to make it, so quite small scale. There is always more to do, but I think this is relevant to the claim of this paragraph. I see that this paragraph has 4 citations for the first sentence alone, which seems like too many, so removing 1-2 of those could make way for citing this relevant work if the authors agree that what we published does in fact do what they say is an important thing to do, even if not perfectly, but still more than anyone else thus far. The journal citation is Brown, R. A., Pasternack, G. B., Wallender, W. W. 2014. Synthetic river valleys: creating prescribed topography for form-process inquiry and river rehabilitation design. Geomorphology 214: 40-55. 10.1016/j.geomorph.2014.02.025. The code is open-source and free to the world presently coded in R as "River Builder". The R package and user's manual can be downloaded from the CRAN website at https://cran.rproject.org/package=RiverBuilder. The code also includes the Perlin function that can create very small scale features, and that is a common method for generating landscape terrains in the video *game and animation industries. In the future we hope to add the capability to parametrize the sub-reach-scale fluctuations in spatial series of detrended bed elevation and lateral topographic breaklines using wavelet parameterization.*

Response:

This is true; we shouldn't ignore these studies because they are relevant and important to this literature. For that we will change the paragraph in the page 2 from the line 3 to 8 by: "Many researchers are working on determining the best simplified representation of channel geometry (Saleh et al., 2013; Grimaldi et al., 2018), based on the variability of cross sections but without the knowledge of the bed elevation variability on a small scale. While other studies focused on the generating of river terrains exactly to specification from the equations and parameters with taking into account the sub-reach scale variability (Brown et al., 2014). Longitudinal variability in river geometry has greater impact on the simulation of the water level than cross-sectional shapes (Saleh et al., 2013) and it must be taken into account in the hydraulic models."

The third paragraph of the introduction serves no required purpose and neither does Figure 1. Both can be deleted with no loss of understanding. Yes, rivers come in different types, but the main thing readers need to know is that this is a study of riffle-pool reaches and that the method can apply to other reaches; these ideas can be promoted without any of this paragraph, as is indicated by the first sentence of the very next paragraph just fine.

Response:

It's true; we should focus on the alternating morphological units especially the riffle-pool sequences without including this paragraph and the figure 1. We will add to the line 14: "In fact, the topographic variability is related to the channel morphology types. In this study, we focus mainly on alternating alluvial channels especially pool-riffle sequences, even though …", and we will remove lines from 9 to 13 and figure 1.

p.2, lines 15-16. The objective of what? The writing is unclear here. I disagree that the main purpose of quantitative analysis of channel topography is just to get pool spacing. In support of our River Builder software, one normally wants to analyze many aspects of reach-scale topographic variability so that they can all be parameterized and used to make realistic synthetic rivers. Other important variables would be parameterizations of thalweg planform curvature, base flow and bankfull channel width undulations, floodplain width undulations, and then how all of these are phased relative to each other (in time series that's "coherence" and "cross-phase"). Thus, pool spacing is certainly one useful data output, but not alone or necessarily most important.

Response:

This is true, the paper isn't about the pool spacing identification, but its purpose is for example extracting some quantitative properties of these alternating morphological units such as the mean and the mode of their longitudinal spacings, with a "continuous" vision of the topography instead of a discrete classification. This will be done by focusing on two kinds of numerical criteria computed at reach scale: The distribution of spacings between morphological units (mean, mode, etc.), and after computing the mean values of geometrical and flow properties (velocity, hydraulic radius, bed shear stress, etc.) in each class of morphological units (e.g. pools, riffles, runs, etc.) the evaluation of the covariance matrix of these parameters.

I also note that tat the authors never use their reach site results to present any conclusions about the science of pool spacing, so if it is so important than its value should be evident in how the results are used to advance science.

Response:

We should be clear in that point that our paper is methodological research that propose a new method with new developments. Of course we will add some conclusions about the longitudinal spacing results and the covariance matrix of the variables chosen.

p. 3, lines 1-4. No need to define wbf twice. Remove one of them.

Response:

We will remove the second one and change the first at line 3 by: "the reach average bankfull width (wbf)".

p. 3, lines 7-17. A major problem with the historic work cited here that its all pre-2001 and how it is presented is that the authors are not addressing the equal importance of channel width undulation to channel depth undulation. Richards in the 1970s understood it and wrote about the importance of width. However, because people didn't tend to make width profiles down rivers, the focus wrongly got limited to depth undulation in the literature of the late 20th century. Of course, authors studying velocity reversal concepts did start to understand this problem pretty well by 1990. With modern high resolution DEMs since 2000, that problem is over and now we are in the era of looking at how depth and width co-vary to control pool and riffle topography and morphodynamics vis-a-vis the "flow convergence routing" mechanism explained by MacWilliams et al (WRR, 2006) and explored further by Prof. Jose Rodriguez in recent WRR papers as well by my lab group in several articles (Sawyer et al., Geomorph., 2010; Brown et al., Env. Man., 2015; Strom et al, Hyd. Proc., 2016; etc). My lab group has published a series of papers on the importance of linked depth and width undulations that has culminated in a new sub-reach scale channel unit classification relevant to this paragraph and this study. See these two articles, the rest leading up to these are cited in them: -Pasternack, G. B., Baig, D., Webber, M., Brown, R. 2018. Hierarchically nested river landform sequences. Part 1: Theory. Earth Surface Processes and Landforms. DOI: 10.1002/esp.4411. -Pasternack, G. B., Baig, D., Webber, M., Brown, R. 2018. Hierarchically nested river landform sequences. Part 2: Bankfull channel morphodynamics governed by valley nesting structure. Earth Surface Processes and Landforms. DOI: 10.1002/esp.4410.

Response:

You're right, we didn't present our literature in that chronological way, which is interesting for the reader, we should focus not only on the identification methods but also on the science of alternating bedforms including pool-riffle, but honestly a methodological paper focusing on alternating bedforms should mention these works and shouldn't neglect any of them. As you suggested, we will change this entire paragraph and summarize it in a table and add studies done after 2000. In addition to that we will present the literature of depth and width undulations in relation to pool-riffle, the same thing for the modern high resolution DEMs, and the thresholds chosen in literature which would help us to discuss our results.

p. 3, lines 20-26. Yes, I agree with all of this, though I don't think wavelet analysis cannot be called "new" as it has been published in geo/hydro journals for decades now; what's new is high quality topo data to apply it to, though that is present intros study. I'm surprised by the citations the authors offer here, as they are not very relevant compared to other options, such as (most importantly) Gangodagamage et al. (Geomorph., 2007) but also Lashermes et al. (WRR, 2007) and McKean et al. (Rem. Sens., 2009). One can use spatially evolutive Fourier analysis and autocorrelation analysis or, if one limits the analysis to a single reach, regular Fourier analysis where the average parameterizations are reasonable.

Response:

What is new is the method presented itself and the identification of alternated morphologies, since it is never made with wavelets. But for the wavelets, yes it has been present for decades, however it is still less used compared to Fourier. Wavelet Transform Modulus Maximum (WTMM, Muzy et al., 1993; Gangodagamage et al., 2007) and other studies using the wavelets in the geomorphological field (Lashermes et al., 2007; McKean et al., 2009) consist in extracting components of the signal, but they are not specifically designed to identify pseudo-periodic components in a univariate, let alone in a multivariate case. For this reason, we introduce the procedure called Wavelet Ridge Extraction (Lilly and Olhede, 2009). These works will be presented in the revised version.

One might even argue that the locations where the Wavelet analysis indicates a change in parameters could be a reach break. Certainly wavelet analysis is a very good way to go for this to objectively delineate reach breaks, but preferably with a multivariate strategy using both depth and width variables. A good comparison would be to look at the riffle-pool quasi periodicity analyses of Brown, R. A., Pasternack, G. B. 2017. Bed and width oscillations form coherent patterns in a partially confined, regulated gravel–cobble-bedded river adjusting to anthropogenic disturbances, Earth Surface Dynamics, 5, 1-20, doi:10.5194/esurf-5-1-2017.

Response:

That's what we will do in the revised version, we will focus only on the wavelet analysis and ridge extraction in the univariate case using one of the three variables (velocity, hydraulic radius, and bed shear stress) and the multivariate case by using the all three of them and compare its results with two benchmark methods: BDT and MEM index.

P.15, line 14. This sentence makes a key determination that flies against the same kind of decision-making applied to the index method of section 4. Specifically, the determination of riffles and pools is going to rely entirely on bed elevation. It seems odd that scientists who begin with the conjecture that *multiple variables should be used to determine riffles and pools would now contradict themselves and only consider one variable. My view of it is that both decisions are arbitrary, as (a) the former was based on a questionably PCA analysis lacking a mechanistic basis and choosing interdependent variables rather than the proper independent ones and (b) the latter is likely based on the amount of work it takes to apply the wavelet methodology and so its application is being limited to only one variables and to only 1 reach instead of all the reaches. I am making my own guess with (b), but the authors provide no justification for limiting the analysis to only 1 reach after introducing so many reaches. Similarly why not do all three variables the authors deem important. A quick check of the scientific literature confirms that multivariate wavelet analysis exists and is available for use. And then there is the issue of how the variables couple to affect riffle and pool occurrence, structure, and resultant processes. The decision-making here is too opaque and needs explanation per these issues. I expect the decisions cannot be justified, but the authors deserve a chance to try.*

Response:

According to these comments:

(a) We totally agree with you that the choice of these 3 variables is unsound, so we will keep the multivariate case with a physical combination of variables. In the revised version, we will focus on the wavelet analysis using the univariate and the multivariate with three variables; we choose the classic three ones: velocity, hydraulic radius, and shear stress.

(b) For the multivariate case, it isn't a problem of computation time, we already have an implementation of the wavelet ridge extraction in a multivariate case; however, we initially chose not to present it in the paper because we need to introduce a specific criterion to identify the local wavenumber K(x). Basically, in the univariate case, wavelet ridge points are those points of the (x,K) plane where the phase of the wavelet $\phi(x,K)$ changes in space exactly at rate K (i.e., $(\partial\phi/\partial x) - K = 0$: the signal is locally similar to a sinusoid of wavenumber K in rad/s). In the multivariate case, we search not for an equality but for a local ***minimization of the norm of the vector*** $[(\partial\phi 1/\partial x) - K ; (\partial\phi 2/\partial x) - K ; (\partial\phi 3/\partial x) - K]$ with respect to K : the local wavenumber K(x) is such that ***<u>all</u> three variables locally look like sinusoids of <u>same</u> wavenumber K(x), but with potentially different phase shifts***. Clearly, this co-evolution is needed to identify morphological units. We will add the necessary mathematical developments in the appendix of the revised paper.

p.5, lines 19-20. This explanation is incorrect on two levels. First, energy gradient is more than just water surface slope, because energy also accounts for the velocity head that is not in that term. Often velocity isn't changing over long distances or is assumed to not change, but along a riffle crest and in the transition to a pool it definitely changes quite a bit, so strictly speaking one has to account for that. Second, the energy gradient is stage dependent, because the steepest gradient is always associated with the vicinity of the smallest cross-sectional area, all other things being equal.

Response:

The complete quotation of the paper by Yang (1971, p. 1567) reads: “For practical purposes, the energy gradient for most natural streams may be replaced by water surface slope without much error”. So we are of course fully aware of the difference mentioned in the referee’s comment, even though this difference between energy slope and water surface slope is usually small for “low Froude” rivers such as the ones studied here. But to avoid this misunderstanding we will refer only to the energy gradient.

At low discharge the way the authors describe it is true, because at low discharge riffles have the smallest XS area. However, once the discharge exceeds the value for the minimum cross-sectional area of the reach to be elsewhere, then it is not at the riffle any more. At some high flow it will become at the pool location, and of course this is the main reason why pools scour and riffles aggrade to maintain relief in alluvial channels, all other things being equal (especially substrate). This stage dependence is a key issue to account for in any scheme to evaluate where riffles and pools are located and it its why considering only depth and ignoring width has always been a mistake by the river science community. Now that we have width data commensurate to depth data, we can move on to the proper treatment of the problem considering their linked co-variance.

p.9, lines 7-8. These claims apply only to low discharges, due to the flow-dependent nature of riffle-pool hydraulics. How they develop as discharge increases depends on the shape of the cross-sections (especially depth vs width “geomorphic covariance”, per flow convergence routing theory.

Response:

Absolutely true, that is why we chose low flow instead of high discharges. We completely agree that the maximum shear stress may be located in different morphological units at high discharges than at low discharges, and that it is very important to understand how relief is maintained. Exploring this near-bankfull behaviour is the reason why hydraulic modeling was needed in the first place in the study by Navratil et al. (2006), since it is difficult to obtain field measurements precisely at bankfull conditions. The wavelet ridge extraction could perfectly be applied at bankfull conditions, but since it would rely on modeling results if we want to perform it on our dataset, we will leave it out of the scope of the paper.

p. 5, lines 22-37. All of these methods retain the limiting viewpoint that they put a primacy on riffles and pools, either ignoring other morphological units (MUs) or treating them as irrelevant. Thankfully, 2D and 3D hydrodynamic modeling ends that mistake and enables objective mapping of all MUs with decision-tree analysis. This approach was explained by Wyrick et al. (Geomorph, 2014) and then applied in Wyrick and Pasternack (Geomorph., 2014) to not only show the greater diversity of MUs beyond riffles and pools, but also to compute simple metrics like pool spacing. Thus, Wyrick and Pasternack (Geomorph., 2014) presented a novel methodology to extract pool spacing from 2D hydrodynamic model outputs of MUs using GIS tools. That is very relevant to this literature review, because it shows recent progress in automated extraction of this metric. The authors are arguing that their methods are more automated and better than pre-existing methods, but they have not actually considered more recent automated methods.

Response:

This study of Wyrick et al. (2014) is relevant and presents a method that should be mentioned in the literature of this paper and also should be discussed in the discussion part.

Meanwhile, the sentences about the outstanding work by Almeida and Rodriguez as well as Parker goes off topic from pool spacing to get into the separate topic of riffle-pool morphodynamics, of which there is a very long and illustrious literature not addressed. Best to cut those at this location and stay focused on the directly relevant literature about pool spacing that is the focus of this study. They may be relevant if the revised manuscript ever addresses processes explicitly.

Response:

True, we should focus only on alternating morphological units and all references linked to morphodynamics or pool-riffle processes should be cut out from the paper.

p.6., lines 5-27. Very good literature review and written well, just not accounting for many recent studies since 2001.

Response:

We will add more recent studies.

p. 7, line 6. The sentence about having surveyed "many" cross-sections is poorly constructed and, in my view, not accurate. Terms like "many" are relative, so it could be that for one person any arbitrarily small number of cross-sections would still seem like many; that makes it hard to argue the point. However, the key metric here is that one cannot analyze for topographically significant spatial frequencies at resolutions smaller than the minimum XS spacing, and that's already quite conservative. For that reason, my lab group uses vastly denser cross-sectional spacing than that used here. For example, in Pasternack et al. (ESPL, 2018b) we used a spacing of 3% of bankfull width. That's "many". For another group, Legleiter (Geomorph., 2014b) spaced a XS every quarter channel width. In contrast, in this study, an analysis of Table 1 finds that cross-sections are spaced between 0.46 to 2.9 times bankfull width, with two reaches not even having 1 XS every bankfull width. These numbers of cross-sections are more like the amount used in a conventional reach survey to obtain reach-average depth and width metrics, not to identify the underlying nature of variability. I think if the authors refer to previously cited articles above about spatial series analysis of rivers topography plus Legleiter, they'll get a better sense of what is needed to get at the detailed patterns of fluvial topo spatial series at the sub-reach scale. This issue doesn't invalidate the study, but just recommends to back off the "many" and get to saying "a normal number of cross-sections typical for a 1D hydraulic modeling study" or something like that.

Response:

We agree with you in that point, the use of "many" in this sentence is relative, so we will change it to "a normal number of cross-sections typical for a 1D hydraulic modeling study".

Also, these cited works could be referred to in the discussion section to help compare and contrast undulation metrics from different studies, including when undulations may not have high enough amplitude to become a "riffle" or "pool" but are still big enough to make a difference for the intermediate morphological units that are mentioned but not investigated in this study.

Response:

We will cite these works in the literature review and we will discuss our work according to it to make some conclusions on the parts where we have pools and riffles without investigating the other MUs and also about longitudinal spacings.

p. 7, line 6. I think a bigger questionmark for the technical soundness has to do with the mindful decision to not have all cross-sections regularly spaced, but to place them primarily at hydraulic controls and morphological breaks. The authors then interpolate to get a grid, but the source data is not uniform. I fully understand why that would be done for a 1D hydraulic modeling study and given perhaps limited resources and no lidar data, but there is no question whatsoever that biased (aka mindful) XS placement impairs and calls into question spatial series analysis as far as objective identification of parameters. By placing the XS where the authors think important hydraulic and morphological things are happening, then necessarily the wavelet analysis and any other method is also forced to bias results toward the same outcome of where significant things are happening. On the other hand, when I put an XS every 3% of bankfull width along the series, then there is no chance anything will be missed and the algorithm can decide for itself what the frequencies, amplitudes, and phases (and other parameters) are for that reach. Equal spacing of XS is the best approach for unbiased results. I think there are some things that can still be analyzed with a small number of mindfully selected XS positions, but I would never take this approach. I do understand the lack of availability of lidar and other remote sensing data to facilitate high-resolution mapping though, but then one has to be thoughtful about what one can reasonably achieve. I think the way forward would be for the authors to explain their viewpoint on why they have a sufficient number of XS for the goals of their study in comparison to the highest density used by the references cited above.

Response:

We fully agree that the larger the number of cross-sections, the more robust identified correlations will be. Unfortunately, we had to use the dataset as it is as we have currently no means of doing additional field work to enrich it. But we do not think that the "biased" placement impairs the overall methodology. Of course we would be pleased to have the opportunity to test this approach on a much denser dataset in the future.

p. 8, section 4, line 1. "Hydrological" should be "hydraulic". I believe. These are not interchangeable. Hydrologic would be rainfall-runoff and water balance related, could be purely discharge but discharge alone does not identify riffles vs pools. Hydraulic means on the basis of the depths, velocities, and other flow kinematics.

Response:

Indeed the correct term is clearly hydraulic in this context.

p. 8, lines 8-16. I am confused by the writing. On line 8 it says hydraulic data were "surveyed" at 3 discharges. Please clarify that the data were measured in the field and then it is necessary to also describe how the data were measured. There are many different methods possible and one cannot undertake analyses of data without stating how it was collected. Moving on from there, if the data was actually measured, then I have absolutely no idea why the authors mention a method involving 1D hydraulic modeling of the sites. Given field observed cross-sections and hydraulic data, once could use a pure XS analyzer like the old, free software WinXSPro and many other GUIs to extract geometric variables like hydraulic radius with no numerical modeling. If the derivative variables like Rh and Fr are not based on field data, but instead are coming from a 1D hydraulic model, then it opens up a whole can of worms regarding the accuracy of the model outputs, which then necessitates an explanation of model calibration and validation performance. All of this is written unclearly and needs to be revised to explain to readers what is going on. This has profound consequences for evaluating the study.

Response:

Data are measured in the field; we will add a description of how it was collected from Navratil et al. (2006): "Cross-sections and water surface profile measurements are surveyed in 2002 – 2004 covering the main channel and floodplain and using an electronic, digital, total-station theodolite. The water surface profiles are measured at different flow discharges."

Reach	Number of cross-sections	Flow discharge surveyed (m3/s)	Gradient
1	14	0.22 and 1.26	0.0125
2	32	1.85 and 2.41	0.0044
3	21	0.18, 1.13, 1.72, and 1.99	0.0018
4	26	0.19, 0.33, 0.8, and 11.5	0.0024
5	25	0.15	0.0060
6	36	0.21	0.0047

The numerical model used in this study aims to calibrate the Strickler coefficient in order to fit the surveyed water surface profiles. In our revised paper, we will solely rely on measurements at the lowest surveyed discharge and use the model to provide estimates of Strickler roughness coefficient K_s, we will use these K_s in order to compute the third degree of freedom, bed shear stress $\tau_b(x)$, along the reach.

Section 4. In the previous section it was stated that hydraulic variables were "surveyed" at 2 low discharges and 1 near bankful discharge. As the relative magnitudes of the variables between rifles and pools are stage dependent, it matters which flow was used to for the analysis in this section. The authors should state that. If somehow all three discharges were used, clarify how. In fact, at-a-station hydraulic geometry is an important tool for identifying riffles and pools more holistically considering the totality of the bankfull channel, so it is too bad few people take note of that and apply it to this purpose.

Response:

We mentioned in the page 8 at line 3 that we worked with the minimum discharge Q_{min}.

p. 8, section 4, lines 4-5. Most people use PCA for challenging multivariate problems with complex interrelationships that are unknown and thus this is the first way to get a sense of how variables interrelate. That does not characterize the situation for riffle-pool geometry and open channel hydraulics. A wiser strategy here could be to use Buckingham Pi theorem dimensional analysis to create the variables of interest. Also, one can easily reason out that really the variables that matter are those that control or respond directly to morphodynamic processes, such as flow convergence routing or meander migration. That can then guide wise variable selection that is process based. Returning to this list of variables, several of these variables are highly correlated or define each other, so it does not make sense to throw them all into one multivariate analysis as if it is a mystery. For example, bed elevation, max depth, and hydraulic radius are all highly correlated and redundant. Meanwhile, A and P define Rh, so those 3 are also highly correlated. Similarly, Fr is defined by y and u, so the same situation arises. This "throw everything into the soup" strategy of multivariate analysis is not wise and possibly not technically sound, but the authors can review the PCA assumptions and limitations to evaluate that- not worth my time to re-study up on PCA. Even if it is technically ok, it still doesn't make any sense as a strategy as if we do not already know how these variables relate to each other- we do know exactly how they relate.

P.9. I am just not understanding why anything in Figure 4 and the associated results text is actually new results or anything other than trivial findings. By definition of variables, A, Rh, and y are positively correlated, while Fr is going to be negatively correlated to y and positively correlated to u. Also, Z has to be negatively correlated to A, Rh, and y. This is all be definition. PCA is not required to know this. Further, I do not agree that the PCA is adding any fundamentally new or useful information for riffle-pool delineation compared to wisely selecting the few independent variables underlying the physics-based analytical relations, especially bed elevation, width, and possibly slope, as together these three control relative velocity between riffle and pool units for a fixed discharge. If the channels are meandering, then thalweg planform curvature would be important, too, as it is well known in the physics to control meander migration. In fact, it is unclear and technically unsound to exclude metrics of channel width from this analysis, as width is the underlying independent variable influencing all the other variables in the list except for detrended bed elevation and depth (which of course are the same thing just inverted and with different vertical datums). The authors need to set up this methodology better to justify why it is necessary and better than what I am proposing as an easier, more process-based approach or else I do not see how this PCA analysis is meritorious.

p.9, lines 3-4. The claim that each descriptor adds additional information about the bedforms is easy to show as not true. Rh is defined by A and P, so how is Rh fundamentally new and additional as opposed to using a combination of A and P, unless one defines the mathematical operation of division as adding new content, which it does not. This continues the theme of my last few comments. The authors are applying blind statistical methods to what is a pure analytical problem with 100% defined and known elements. There is no additional information beyond the independent variables and the math operators to combine them into A, Rh, and Fr.

Response:

In the initial version of the paper, we were comparing the wavelet-based method with two benchmark methods: the BDT (O'Neill and Abrahams, 1984), and an index method that consists in affecting a different numerical value for each class of a given variable/degree of freedom, and then sum these individual index functions into a composite one.

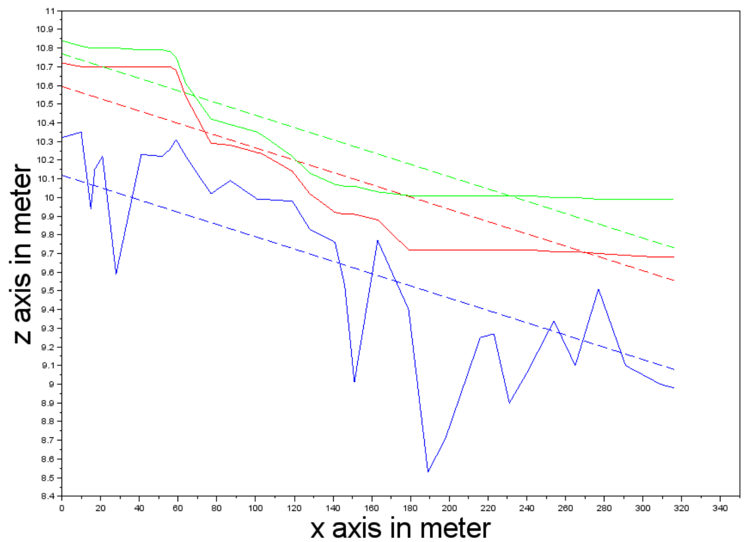
The major concerns are not about the index method in itself, but on the choice of the variables/degrees of freedom. Initially we used the first three axes of a Principal Component Analysis as the degrees of freedom, a choice which has very poor physical meaning. We will entirely change this choice and build the index using the same variables/degrees of freedom as in the existing "Mesohabitat Evaluation Model" (MEM, Hauer et al., 2009), which uses velocity, hydraulic radius (or the closely related cross-sectional averaged depth), and bed shear stress. We will also use the same threshold values for classifying each variable.

p. 8, section 4, line 8. The topic of detrending is a huge issue that requires a bit of unpacking in the writing here, because the outcome of riffle-pool delineation can be largely depending on this very choice based on my own sensitivity analysis of this situation using different detrending methods. Earlier in the manuscripts the authors wisely commented about all the different way different authors measure and analyze pool spacing data (e.g., p.6, line 22). Well, the same challenge arises with detrending. There is no universally right or wrong way given the diversity of purposes for detrending, but each option has consequences for the scientific outcome for a specific purpose, especially for identifying the magnitude and length of residual highs and lows in a bed profile. Without going into all the options, what I request is that the authors state what type of detrending they did. If linear, then was it one line per site (presumably no reach breaks within a site, but there could be) and was care taken to insure that the line began and ended at the same relative elevation to avoid biasing the slope, which is a significant problem.

Response:

The procedure that we followed to construct the detrended bed elevation relies on the bed elevation and all water surface levels to avoid biasing the slope. For example, given a reach with N surveyed cross-sections in two discharge stages $Q_1$and Q_2, we define the bed elevation as $z_{talweg}(x)$ and the two water surface levels $z_{ws}(x, Q_1)$ and $z_{ws}(x, Q_2)$. For the detrended bed elevation according to that, it is $Z = z_{talweg} - \mathrm{z}_{trend}$, where $\mathrm{z}_{trend} = \mathrm{S} \times x + b_t$, S and b_t are solutions of the system below.

$$
\begin{bmatrix}
x_1 & 1 & 0 & 0 \\
x_2 & 1 & 0 & 0 \\
\vdots & \vdots & 0 & 0 \\
x_N & 1 & 0 & 0 \\
x_1 & 0 & 1 & 0 \\
x_2 & 0 & 1 & 0 \\
\vdots & 0 & \vdots & 0 \\
x_N & 0 & 1 & 0 \\
x_1 & 0 & 0 & 1 \\
x_2 & 0 & 0 & 1 \\
\vdots & 0 & 0 & \vdots \\
x_N & 0 & 0 & 1
\end{bmatrix}
\begin{bmatrix}
S \\ b_t \\ b_{Q_1} \\ b_{Q_2}
\end{bmatrix}
=
\begin{bmatrix}
z_{\text{talweg}}(x_1) \\
z_{\text{talweg}}(x_2) \\
\vdots \\
z_{\text{talweg}}(x_N) \\
z_{\text{ws}}(x_1, Q_1) \\
z_{\text{ws}}(x_2, Q_1) \\
\vdots \\
z_{\text{ws}}(x_N, Q_1) \\
z_{\text{ws}}(x_1, Q_2) \\
z_{\text{ws}}(x_2, Q_2) \\
\vdots \\
z_{\text{ws}}(x_N, Q_2)
\end{bmatrix}
$$



As mentioned in the figure, the bed elevation is in blue, water surface for Q_1 is in red and for Q_2in green, the stippled lines are the trends.

p.9, lines 10-12. This single long sentence attempting to explain a sequence of mathematical steps applied to some data is opaque to me as a reader, as is plot (a) in Figure 5. This should be written out more thoroughly and clearly in steps. For example, presumably the smoothed data is each XS spatial series, but then what constitutes the "sampling" that is "homogenized"? I neither understand the samples nor what homogenization is and why it is needed. Is homogenization the same or different from normalization in this study? If so, why call it two different things that creates reader confusion, but if not then what is it? Sometimes normalization means the strict application of the function that makes the data fit the normal probability distribution while more often it just means to divide variable by another.

Response:

This data goes through some processes; first, detrending the variables (as bed elevation), then sampling all variables, this process is a linear interpolation with spacing of 1m or smaller. Second, normalizing and centering them which are just the variable minus its mean divided by the standard deviation. The formulation of smoothing was wrong; this treatment that we performed is a general removal of very low frequency components (wavelength larger than 7 times the mean bankfull width) before applying thresholds. Since we will not use bed elevation anymore in the index method (MEM), this processing is no longer relevant.

P. 10, first line. Why is this line bold?

Response:

It's what's concluded from the previous paragraph which defined the index. As said before, this part will be dismissed.

P. 10, equation (5). This equation is an all-or-nothing type approach where every location is either classified as riffle or pool for an individual descriptor. This is in contrast to the aforementioned BDT approach that uses a standard deviation tolerance. Also, the method of Pasternack et al. (ESPL 2018a,b) uses a standard deviation tolerance. It would be useful to explain why no tolerance was applied.

Response:

This section will be completely removed so this discussion is not relevant anymore.

P.10, line 10. From what I gather considering the equations and the potential values of I, the concept here is that for something to be defined as a riffle or pool versus an intermediate MU type, all three descriptor variables must agree and yield the same heavyside function value of 0 (pool) or 1 (riffle). Conceptually, the authors are substituting a cross-check among 3 variables as the countermeasure to cope with uncertainty in place of tolerance within each variable as the countermeasure for uncertainty. I think putting the concept of the method in words like this would help readers understand the strategy and purpose of the math and procedure that is described. However, looking beyond that, one can ask if this actually works? In other words, is there a resiliency against uncertainty gained by using multiple variables and the specific ones chosen?

The authors should address why they think this is so, because this is the kernel of new idea they are proposing but have not actually written out. I have to agree that using more than 1 INDEPENDENT variables would help serve as a check against uncertainty, so that is good idea, but (a) the variables chosen are not independent (both Fr and Rh depend on detrended bed elevation, which is a surrogate for the inverse depth and depth goes into both Fr and Rh) and (b) one can choose to use both a tolerance per BDT and multiple variables per this study. That would yield the best outcome. In Pasternack et al. (ESPL, 2018a,b), we do use both strategies, but for our choice of variables we limit our analysis to only detrended bed elevation and width, as these are the process-based controls on flow convergence routing, they are independent, and they underlie the derivative variables like Fr and Rh. However, we do not use slope, which independently controls velocity and Fr, and we make that choose for a specific process-based reason, but we do exclude it. We also do not look at thalweg planform curvature in those articles, though we have internally thus far. One could reasonably choose to include both slope and thalweg planform curvature. One could also choose to include grain size metrics, as I'm sure prof. Jose Rodriguez would be very insistent on given the importance of that variable to determining relative erosion and deposition on riffle sand pools. Unfortunately, it is incredibly difficult to obtain high-resolution spatial series of substrate grain size as of yet. In any case, I see both positive and negative to what is being done.

At a minimum, the authors can explain the general idea in words as I have done, but then also some defense is needed if the authors stand by the decision of variables chosen, because I see the choice as technically unsound given that they are defining each other as explained.

Response:

As I said before, for the choice of these methods, we will cut out the index method and replace it with an existing method "Mesohabitat Evaluation Model (MEM)" inspired from Hauer et al., (2009) applied it to three independent variables (velocity, hydraulic radius, and bottom shear stress). For that, we will focus only on the wavelet analysis and ridge extraction in the univariate and the multivariate cases and comparing its results with two benchmarking methods: BDT (O'Neill and Abrahams (1984)) to the bed elevation data and MEM (Hauer et al., 2009) to the three variables.

P.11, line 2. I see that p.6 line 10 defined lambda-star as "dimensionless pool spacing", yet here that variable has dimensions of m? Something is wrong.

Response:

It's a typo! There is no "m".

p.12, lines 1-2. While most people only apply Fourier analysis to stationary series, the method is not in fact limited as thus, because it can be applied using the "evolutive" methodology to capture non stationary dynamics very similar to what one gets from wavelets. One can reasonably argue that wavelets are superior for non stationary data and because one can apply different wave forms, but to say that Fourier analysis cannot do non stationary analysis is wrong. Many applications of evolutive Fourier analysis exist, but for hydrological data see for example, Pasternack, G. B. and Hinnov, L. A. 2003. Hydro meteorological controls on water level in a vegetated Chesapeake Bay tidal freshwater delta. Estuarine, Coastal, and Shelf Science 58:2:373-393.

Section 5.1. I think there is too much redundancy between what was written about wavelets in section 1 (p. 3, lines 20-26) and this section. The introduction can more simply introduce the idea of it and state the scientific questions and hypotheses associated with using it, but then leave the literature review here, so there is only one literature review. My earlier comments about the literature of applying wavelets to geo/hydro data also apply to this section.

Response:

We will change these paragraphs to:

"Classical mathematical methods, such as Fourier analysis, extract the wavelengths in the frequency domain for stationary signals but also for nonstationary signals using an "evolutive" methodology based on spectral estimators (Thomson., 1982; Pasternack and Hinnov., 2003). Wavelet transforms can do the same for nonstationary signals and find the localized wavelength but with different waveforms. Analyzing a signal basically consists of looking for similarity between the signal and well-known mathematical functions. In this paper, we use the continuous wavelet transform with the Morlet wavelet

(Gabor, 1946) applied to spatial series instead of time series, so periods and frequencies in time series are replaced by wavelengths and wavenumbers in spatial series.

The wavelet transform is done by convolving the mother wavelet (the waveform) with the signal data, which begin first with the product of the wavelet and a portion of the signal and that product is then integrated to define a mathematical measure of similarity of that portion of the signal to the reference wavelet. This process is repeated as the mother wavelet is moved along the signal and also dilated or contracted to different spatial scales. Thus the transform is done in space and scale (or frequency) simultaneously (McKean et al., 2009)."

p. 15, line 15. Why choose the Orgeval reach, when it is not the longest or having the most XSs? I already deleted my table where I computed the XS density, so does this reach have the highest XS density? Otherwise, why? of course, why not analyze and compare all 6 reaches, as this is a scientific journal article and there could be interesting results in comparing the different reaches? The method itself of applying wavelet analysis to a spatial series is not so novel as to justify limiting to only 1 reach as a single case study.

Table 4. I do not understand. Previously it was stated twice that only 1 reach was assessed but now here are data comparing all six reaches. I think the writing of the manuscript should be improved to explain what is going on better. If all six reaches were in fact tested with wavelet analysis, then some comparison between reaches would be interesting for section 5.

Response:

As I said in the paper this is just a reach example. In the revised paper we will present all the 6 reaches.

Figure 8. This figure shows a fundamental problem with the wavelet methodology as the preferred tool for mapping riffle and pools as well as quantifying their spacings. Specifically, it cannot return results for some distance at the start and end of the spatial series. In the case shown, there is only results for the range of ~ 81-241m out of 318 m. That leaves a whopping 50% of the reach unassessed. Wow. That's a lot of lost information. Of course, the longer the series and the more frequent the XS sampling, the less loss, but there will always be a loss. This makes the method less valuable than alternatives that retain the information.

Section 6.2 This section now states that the comparison is limited to only 81<x<241 m. That's problematic because it's not a fair test of the actual utility of the wavelet method leaving half the reach unevaluated. This should be stated clearly. The comparison is still useful but it does have this huge caveat. A method that leaves half the reach unevaluated can never be better than one that assess the whole reach, if the goal is to characterize the whole reach.

Section 6.1. Authors must clarify if the score technique is applied to the entire reach length or only the length for which results overlap. I think one must count the whole reach as it is a deficiency of the wavelet method that it leaves 50% of reach 6 unevaluated. Whatever the authors are doing, they should clarify that.

Response:

The origin of this problem is the Cone of Influence; it is the region of the wavelet spectrum in which edge effects become important.

Of course we can say the same think for reach length and number of morphological units as for the number of cross-sections: the larger it is, the more robust the results will be, and the smaller the relative portion of “unassessed length” will be. Edge effects due in the cone of influence are clearly a limitation of the wavelet analysis, since it is an analysis in space and scale (x,k) simultaneously. The method has the drawbacks of its advantages, it still remains a powerful tool for non-stationary analysis. Some authors choose to pad the data series with zeros in order to get results on the whole available length. We chose not to use such a padding, which may introduce bias. We prefer a shorter series of local wavenumber K(x), than a longer, potentially biased one.

p. 17, results header. Some authors like to blend methods and results in paired couplets working through a manuscript, and that is most appropriate when one couplet build son the results of another, but then one would not call section 6 here a results section, as many results have already been presented. If I was the associated editor for this manuscript, I would require the authors to separate the methods content from the results content and go with the traditional ordering of the scientific method, because there is no reason not to. One can state the methods from sections 4 and 5 in one unified methods section and then state the results in a unified results section. As the two sections do not build on each other, then one does not need to use the couplet approach. Then, one can have methods and results subsections for the inter comparison analyses. Finally, discussion should stand alone after all results are presented.

Section structure. I think there are problems with the way the manuscript’s sections are structured. In general I can follow what the paper is trying to do, but the structure would be better following a traditional scientific method with all methods first then all results second, ands then all actual discussion last. By mixing them all up it is somewhat confusing and more importantly, impossible to tell what methods have answered what important scientific questions. For example, from the structure it is difficult to tell if this study is only a methodological comparison or also a scientific contribution presenting new results about pool spacings that can be compared with the results of other studies. It would be a shame to do all this work and have no contribution to the question of pool spacing in different river types. But getting back to my main concern here, the discussion, if present at all, iOS hidden in bits throughout the manuscript and would work better if isolated and thoroughly presented.

Response:

That’s true, for that we will reorganize as presented before.

Discussion section 6.3. These paragraph primarily consistent of more results not previously present, but there is a bit of discussion, too. Specifically, all the text in this section from page 19 line 18 to page 21 line 20 are purely results. In fact, p. 21 line 10 even says, “these results: : :” so the authors view these as results too. Really, there is no suitable discussion putting the results of this study into the larger context *of methods and results about riffle-pool ID-ing and quantifying their spacings. There should be such a discussion.*

Response:

These results will be transferred to the discussion section. The part where we said “these results” is just an error of wording. In the discussing part we will discuss also results of cross-section spacings and their influence on the results according to the Pasternack et al. (2018b) and Legleiter (2014b).

References:

Brown, R. A., Pasternack, G. B., Wallender, W. W. 2014. Synthetic river valleys: creating prescribed topography for form-process inquiry and river rehabilitation design. Geomorphology 214: 40-55. 10.1016/j.geomorph.2014.02.025

Brown, R. A., Pasternack, G. B. 2017. Bed and width oscillations form coherent patterns in a partially confined, regulated gravel–cobble-bedded river adjusting to anthropogenic disturbances, Earth Surface Dynamics, 5, 1-20, doi:10.5194/esurf-5-1-2017.

Gabor, D.: Theory of communication. Part 1: The analysis of information, Journal of the Institution of Electrical Engineers-Part III: Radio and Communication Engineering, 93, 429–441, 1946.

Gangodagamage, C., Barnes, E., & Foufoula-Georgiou, E. (2007). Scaling in river corridor widths depicts organization in valley morphology. Geomorphology, 91(3-4), 198-215.

Grimaldi, S., Li, Y., Walker, J., and Pauwels, V.: Effective Representation of River Geometry in Hydraulic Flood Forecast Models, Water Resources Research, 54, https://doi.org/10.1002/2017WR021765, 2018.

Hauer, C., Mandlburger, G., & Habersack, H. (2009). Hydraulically related hydro-morphological units: description based on a new conceptual mesohabitat evaluation model (MEM) using LiDAR data as geometric input. River Research and Applications, 25(1), 29-47.

Lashermes, B., & Foufoula-Georgiou, E. (2007). Area and width functions of river networks: New results on multifractal properties. Water Resources Research, 43(9).

Legleiter, C. J. (2014). A geostatistical framework for quantifying the reach-scale spatial structure of river morphology: 2. Application to restored and natural channels. Geomorphology, 205, 85-101

Lilly, J. M. and Olhede, S. C.: Wavelet ridge estimation of jointly modulated multivariate oscillations, in: Signals, Systems and Computers, 2009 Conference Record of the Forty-Third Asilomar Conference on, pp. 452–456, IEEE, 2009.

Lilly, J. M., & Olhede, S. C. (2010). On the analytic wavelet transform. IEEE Transactions on Information Theory, 56 (8), 4135–4156.

McKean, J., Nagel, D., Tonina, D., Bailey, P., Wright, C. W., Bohn, C., & Nayegandhi, A. (2009). Remote sensing of channels and riparian zones with a narrow-beam aquatic-terrestrial LIDAR. Remote Sensing, 1(4), 1065-1096.

Muzy, J. F., Bacry, E., & Arneodo, A. (1993). Multifractal formalism for fractal signals: The structure-function approach versus the wavelet-transform modulus-maxima method. *Physical review E*, *47*(2), 875.

Navratil, O., Albert, M. B., Herouin, E., & Gresillon, J. M. (2006). Determination of bankfull discharge magnitude and frequency: comparison of methods on 16 gravel-bed river reaches. Earth Surface Processes and Landforms: The Journal of the British Geomorphological Research Group, 31(11), 1345-1363.

O'Neill, M. P., & Abrahams, A. D. (1984). Objective identification of pools and riffles. Water resources research, 20(7), 921-926.

Pasternack, G. B., & Brown, R. A. (2013, January). Ecohydraulic Design of Riffle-Pool Relief and Morphological Unit Geometry in Support of Regulated Gravel-Bed River Rehabilitation. In Ecohydraulics (p. 337).

Pasternack, G. B., & Hinnov, L. A. (2003). Hydrometeorological controls on water level in a vegetated Chesapeake Bay tidal freshwater delta. Estuarine, Coastal and Shelf Science, 58(2), 367-387.

Pasternack, G. B., Baig, D., Weber, M. D., & Brown, R. A. (2018). Hierarchically nested river landform sequences. Part 2: Bankfull channel morphodynamics governed by valley nesting structure. Earth Surface Processes and Landforms, 43(12), 2519-2532.

Richards, K.: The morphology of riffle-pool sequences, Earth Surface Processes and Landforms, 1, 71–88, 1976a.

Saleh, F., Ducharne, A., Flipo, N., Oudin, L., and Ledoux, E.: Impact of river bed morphology on discharge and water levels simulated by a 1D Saint–Venant hydraulic model at regional scale, Journal of hydrology, 476, 169–177, 2013.

Thompson, D. M.: Random controls on semi-rhythmic spacing of pools and riffles in constriction-dominated rivers, Earth Surface Processes and Landforms, 26, 1195–1212, 2001.

Wheaton, J.M., Pasternack, G.B., Merz, J.E., 2004a. Spawning habitat rehabilitation - 1. Conceptual approach & methods. International Journal of River Basin Management 2(1), 3-20.

Wyrick, J. R., Senter, A. E., & Pasternack, G. B. (2014). Revealing the natural complexity of fluvial morphology through 2D hydrodynamic delineation of river landforms. Geomorphology, 210, 14-22.

Yang, C. T. (1971). Formation of riffles and pools. Water Resources Research, 7(6), 1567-1574.